publication is a frequent and a grossly careless error, for in every case the information is forthcoming. Examples are :—

P. 225.—Siddall, NATURE, vol. xv.—instead of *Annual Report Chester Soc. Nat. Sci.*

P. 230.—Williamson, NATURE, vol. xvii.—instead of *Proc. Manchester Lit. Phil. Soc.*

P. 250.—Suess, *Quart. Journ. Geol. Soc.*, xxvi.—instead of *Verh. k. k. geol. Reichs.*

Moreover, the hopeless nature of his published errata may indicate that the author was somewhat ashamed of his work, and it is difficult to understand why the book was not stopped and reprinted, before it was allowed to pass into circulation.

Enough has now been said of the original work—that is, the first attempted list ; we will now pass on to the supplement I. In this, at least, we might have hoped that the compiler would have profited by experience, and used more care. There is certainly a difference in the proportion of typographical errors, but such details as volumes are still rather wild (*Bull. Soc. Géol. France*, for 1886, is quoted here and there as vol. x., xiv., &c.). We gather from the perusal of his supplement many things we could not understand in the original work. We recognize that the compiler is neither a born bibliographer, nor acquainted with scientific literature. We observe with satisfaction that the words "[not seen]" occur more frequently than in the earlier work, but can it be possible that the author has seen a copy of Silvestri's paper noted on p. 62 ? It is exceedingly rare, it does not exist in English libraries, and the writer of this has only seen two copies, both of which were sent to him from Italy. It would have been interesting to learn the pagination of so scarce a paper : the title as it at present stands is strongly suggestive of a bookseller's catalogue. And surely it was worth the compiler's while to quote Ehrenberg properly (p. 65) while the book was presumably lying open before him ? The book also is known as "*Monatsbericht*" not "*Verhandlungen*," that is a secondary title. A very careless error is seen on p. 57, where *Orbitolina conoidea*, Alb., and *O. discoides*, Alb., are quoted. The original gives Albin Gras as the authority, whose paper on the subject, moreover, is well known. There should not have been confusion here. On pp. 64, 65, 71, 72, and 74, the same careless duplication of entries occurs as seen in the first attempted bibliography. But worse than all, perhaps, is the rendering of different versions of the title of one publication. A good instance of this is seen on pp. 66, 67, where six variants of *Verh. k. k. geol. Reichs.* are used, some (*Ver. K. K. Geol.*) being quite unintelligible to the uninitiated. On p. 72 we see two versions of *Ann. Soc. Belge Microsc.*, and only those familiar with the book would recognize readily "*Vierteljahrsschrift d. Zür. Natur. Gesellsch.*" (p. 74), with its chief word abbreviated. The compiler should remember that there is no necessity to quote, but, if he quotes, he should quote correctly.

It is needless to waste space on such clumsinesses as Prof. Wm. King, S.C.D. (? D.Sc.) (p. 1), or *Jahrbuch. Geol. Reicht.* It is also advisable to have some method even in printing. The compiler of this list uses roman and *italics* indiscriminately for titles of works (p. 71, Steinmann—where more prominence is thus given to the review than to the original work), while on p. 63, in the entry Alth, the word *Rozprawy* begins the title of the book, and has nothing whatever to do with the title of Dr. Alth's paper.

Many of these errors and defects might have been avoided had the compiler been accustomed to public libraries, or even endeavoured to find out the common books of reference, always at hand in these places. No bibliographer should ever think of working in scientific literature without his Carus and Englemann, his Scudder, and his Bolton, and for an American to omit to do so is sinful. No greater mistake was ever made by a writer than that made by the compiler, when he wrote in his preface that he had enjoyed facilities not enjoyed by many scientific students, those facilities afforded by the great public libraries of New York. We know what the resources of those libraries are, and the production which calls for this letter does not shake our faith in them. "Instructive" this bibliography certainly is, but not in the sense intended by its compiler.

CHAS. DAVIES SHERBORN.

Density and Specific Gravity.

MAY I ventilate a point in mechanical definition which has perplexed students within my experience—the use of the words density and specific gravity?

We are usually told that the quantity of matter in a body—as it is now called, the mass of the body—is proportional to the volume and density conjointly. This is Newton's definition of density (see also Thomson and Tait's "Natural Philosophy," § 208). Thus, if M be the mass, V the volume, and ρ the density of a body, we have—

$$M = \rho V \quad . \quad . \quad . \quad . \quad . \quad (1)$$

if the unit of mass be taken as the unit of volume of a substance of standard density.

Again, we are told that specific gravity is the ratio of the weight of the given body to the weight of an equal volume of some standard substance (Besant's "Hydrostatics and Hydrodynamics," § 13). Since weights are simply proportional to masses, it follows that the numerical values of specific gravities and densities are exactly the same. It would seem better, under these circumstances, to use one word only to express the one physical property. Accordingly, we find that specific gravity is disappearing from many of our best books (I think from Thomson and Tait's "Natural Philosophy," for example), though it still holds its place to puzzle students in examinations, and therefore teachers are compelled to make the best of it they can.

But this is not the whole evil. The definition of specific gravity is usually followed by the equation—

$$W = sV \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where W is the weight, s the specific gravity, and V the volume of the body. This equation is, no doubt, usually accompanied by the caution that the unit of weight chosen is not the unit of force proper to other dynamical equations, and for this reason the equation

$$W = \rho V g \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is far to be preferred.

If equation (2) is of practical value, would it not be as well to define specific gravity in accordance with it, and say that specific gravity is the weight of unit volume of the substance? Thus, the specific gravity of water would be expressed by 62·5 lbs. avoirdupois in British units, or by 1 gramme in C.G.S. units. I believe this would have the advantage of conveying a perfectly definite idea to minds which dislike such abstractions as mass and density.

L. CUMMING.

Rugby, March 31.

"Coral Formations."

MR. MELLARD READE last week (April 5, p. 535) pointed out an error in my calculations which I had myself discovered when too late, and had intended to correct in sending you a further note on some experiments which are now in progress.

Mr. Reade seems to make use of my arithmetical blunder, and apparently attempts to discredit my experiments, and the new views as to coral-reef formations ; but I leave the matter to those who have a practical knowledge of the subject.

The corals experimented upon were of the class known as hard corals, and consequently the amount dissolved must be much smaller, I imagine, than that dissolved from the softer varieties, such as Porites. The first experiment (p. 462) gives the highest result, but I have no reason to doubt that the rate of solution deduced therefrom is far below that actually taking place in the tropical areas of the Pacific and Indian Oceans.

I do not consider that Mr. Reade has given an answer to Mr. Irvine's pertinent question, though he would have it appear that an answer is patent to everyone, and he must not take up your space with such a trivial matter.

Mr. Murray, speaking of his tow-net experiments in his Royal Institution lectures, says :—"I give this calculation more to indicate a method than to give even the roughest approximation to a rate of accumulation of deposits. The experiments were too few to warrant any definite deductions" ; and he is evidently satisfied that we have no knowledge, other than relative, as to the rate of accumulation of calcareous deposits.

It is at once evident to all who have used the tow-net, that Mr. Murray's experiments afford a very slender basis for calculations. Probably not more than one-fourth of the water in the track of the tow-nets actually passed through the nets, and not more than one-half of the organisms that entered them were retained ; the Coccospheres, Rhabdospheres, and small Foraminifera, for instance, passing through and escaping with the

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water. Then, Mr. Reade supposes all the organisms in the bulk of water taken to die and fall to the bottom each day. Mr. Murray, in his calculations, supposes only one-sixteenth part to die each day. From the same data the former makes out a rate of accumulation of deposit of 1 inch in 29 years, the latter a rate of 1 inch in 470 years. Dana estimates the growth of a reef at not greater than one-sixteenth of an inch in one year, *i.e.* 1 inch in 16 years. Yet it will be admitted that a reef must grow much more rapidly than a deep-sea deposit. What then would justify us in accepting these figures as in any way representing what is now taking place in Nature? The fact is we much want definite information on the rate of growth of these calcareous deposits, and if Mr. Reade has the information his language would warrant, he should make it known for the benefit of science.

We know that these deposits do accumulate to hundreds of feet in thickness in some places, notwithstanding solution; and it seems to me that, as we can imitate in the laboratory the conditions of solution while we cannot those of secretion by organisms, then by experiments in this direction we may at least arrive at a knowledge of the minimum rate of accumulation of oceanic calcareous deposits. JAMES G. ROSS.

14 Argyll Place, Edinburgh, April 14.

Bernicle Geese on Coniston Lake.

THIS afternoon while walking by this lake I saw four large birds flying overhead. These birds, after making several circuits in the air, pitched on the lake. I had with me an excellent pair of field-glasses, and as I succeeded in approaching within 20 yards of them, I was enabled to examine them with sufficient accuracy to convince me that they were Bernicle geese (*Anser leucopsis*, Yarrell). What struck me as most worthy of remark was their extreme tameness, as they allowed me, first on land, and then in a boat, to approach within 20 yards of them. They were in excellent plumage, and seemed in good condition. After remaining about three hours swimming about on the lake, they rose, and after circling round once or twice, flew off in a northerly direction.

May I ask if this is a rare bird to see in the Lake District at this time of year? I have inquired in the neighbourhood, and do not think they could have come from any private water. Several people who have been here for many years assure me they have never seen this bird on the lake before, and this has certainly been my own experience. Is it possible their extreme tameness was due to fatigue? WILLIAM R. MELLY.

Tent Lodge, Coniston Lake, Lancashire, April 8.

The Muzzling of Oysters.

THIS practice, described in the current number of NATURE (p. 572) as owing "its existence to a careful study of the habits of the bivalve," is by no means new, though probably original on the part of the American naturalists. Our London fishmongers have muzzled oysters on a large scale from a time that is immemorial among them. Barrelled oysters are all very carefully muzzled, but without wires, as anybody may learn by watching an expert in the process of barrelling. It will be seen that he lays the oysters one by one carefully in tiers up to the top of the barrel, and then lays another tier rising *above* the level of the top. Having done this, he places the lid of the barrel on this exuberant tier, and thumps and rattles the barrel on a stone pavement or other solid ground until, by close packing of the whole, it descends to the level of the barrel top. The mass of oysters being thus compressed so as to render the slightest gaping of any one quite impossible, he firmly nails down the head of the barrel.

Experience has proved that oysters thus effectively muzzled may take long slow journeys (as they did in the old coaching days) and be kept fresh and without loss of flavour for two or three weeks, provided the barrels are unopened. If, however, they are loosely barrelled, a few days are too many. In some old country houses the barrels, unopened, were placed in salt water, and thus kept until required, but whether this was advantageous I cannot say.

W. MATTIEU WILLIAMS.

The Grange, Neasden, April 13.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

I.

I.—PROBABLE ORIGIN OF SOME OF THE GROUPS.

I. NEBULÆ.

IN a paper communicated to the Royal Society on November 15, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebula lines are coincident in position with the fluting and lines visible in the bunsen burner when magnesium is introduced, and that the fluting is far brighter at that temperature than almost any other spectral line or fluting of any element whatever.

I suggested that the association or non-association of hydrogen lines with the lines due to the olivine constituents of the meteorites might be an indication of the greater or less sparseness of the swarm, the greatest sparseness being the condition defining fewest collisions, and therefore one least likely to show hydrogen. This suggestion was made because observations of comets and laboratory work have abundantly shown that great liability to collision in the one case, and increase of temperature in the other, are accompanied by the appearance of the carbon spectrum instead of the hydrogen spectrum.

The now demonstrated meteoric origin of these celestial bodies renders it needful to discuss the question in somewhat greater detail, with a view to classification; and to do this thoroughly it is requisite that we should study the rich store of facts which chiefly Sir William Herschel's labours have placed before us regarding the various forms of nebulæ, with the view of ascertaining what light, if any, the new view throws on their development.

To do this the treatment must be vastly different from that—the only one we can pursue—utilized in the case of the stars, the images of all, or nearly all, of which appear to us as points of light more or less minute, while, in the case of the nebulæ, forms of the most definite and, in many cases, of the most fantastic kind, have been long recognized as among their chief characteristics.

It will at once be evident that since the luminosity of the meteorites depends upon collisions, the light from them, and from the glow of the gases produced from them, can only come from those parts of a meteor-swarm in which collisions are going on. Visibility is not the only criterion of the existence of matter in space; dark bodies may exist in all parts of space, but visibility in any part of the heavens means, not only matter, but collisions, or the radiation of a mass of vapour produced at some time or other by collisions. The appearances which these bodies present to us may bear little relation to their actual form, but may represent merely surfaces, or loci of disturbances.

It seemed proper, then, that I should seek to determine whether the view I have put forward explains the phenomena as satisfactorily as they have been explained on the old ones, and whether, indeed, it can go further and make some points clear which before were dark.

To do this it is not necessary in the present paper to dwell at any great length either on those appearances which were termed *nebulosities* by Sir William Herschel or on irregular nebulæ generally; but it must be remarked that the very great extension of the former—which there is little reason to doubt will be vastly increased by increase of optical power and improvement in observing conditions and stations—may be held to strengthen the view that space is really a meteoritic plenum, while the forms indicate motions and crossings and interpenetra-

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[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S.

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